

as its position-angle; its form has, however, considerably changed in the interval of time between the exposures. Mr. Fox kindly forwarded me a photograph of this prominence, which is here reproduced (Fig. 2), and he oriented it in the corner on rather a small scale. This orientation is reproduced by me on a larger scale in white

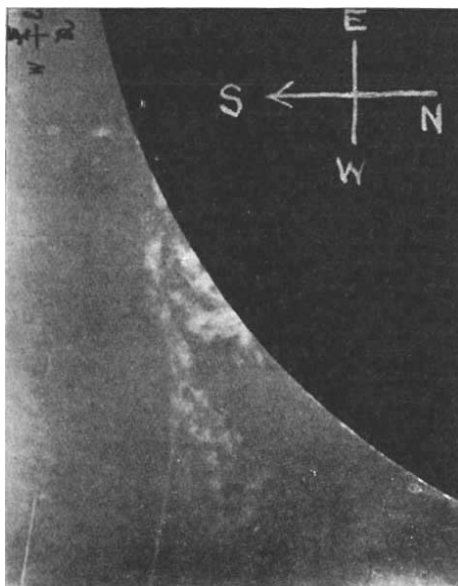


FIG. 2.—The large prominence in the south-west quadrant, photographed in calcium light by Mr. Philip Fox on July 17, 1907, at 5h. 56m. G.M.T., at the Yerkes Observatory, U.S.A.

on the photograph. It will be noticed that the upper portion of the prominence is directed from the south towards the west, but in the Kensington photograph (Fig. 1), and also in Prof. Hale's (Fig. 3), the material is directed from the west towards the south. The question arises, is Mr. Fox's orientation right (his position-angle is correct), or has the material altered its position between the times

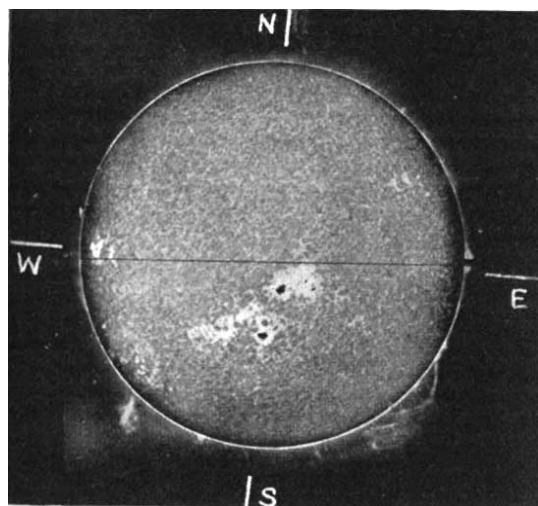


FIG. 3.—The Sun's disc and prominences on the limb, photographed in calcium light by Prof. Hale on July 17, 1907, at 2h. 46m. p.m. G.M.T., at Mount Wilson, U.S.A.

the photographs were taken? I am rather inclined to question the orientation.

Directing attention now to the triple arch prominence about position-angle 137° , which was shown in the first of the Kensington negatives (Fig. 1), I pointed out that in the second photograph, taken thirty-six minutes later,

only remnants of the system remained. So rapidly did the whole of this disturbed region wane in intensity that it is not astonishing to hear that two hours later Mr. Fox reported that no prominence of unusual form was there.

The recent communication to this Journal (vol. lxxviii., p. 151, June 18) by Mr. A. A. Buss is of great interest to me, because it made me acquainted with a photograph, secured by Prof. Hale, of the same prominence taken half an hour previous to the first Kensington picture. Through the courtesy of Mr. Newbegin, jun., I received a copy of this very interesting photograph, and one is now able to follow more accurately the sequence of events in the disturbed area. I hope Prof. Hale will forgive me for reproducing his photograph here (Fig. 3), but it is only by showing the two photographs together that a satisfactory comparison can be made. I have ventured to insert the orientation on Prof. Hale's photograph in order to render the identification of the prominences more easy.

The most striking difference between the two photographs is, apart from their form, the great intensity of the large prominence in the south-east quadrant in Prof. Hale's picture (2h. 46m. p.m. G.M.T.) and its comparative faintness in that obtained at Kensington (3h. 14m. p.m. G.M.T.); other prominences are of about the same intensity in each. This diminution of brightness indicates how rapidly the prominence must have waned during the twenty-eight minutes' interval between the exposures. The second photograph taken at Kensington (3h. 50m. p.m. G.M.T.) showed that this rapid waning had continued.

According to Mr. Buss's visual observations, made between 1h. 30m. and 2h. 20m. p.m. G.M.T., *i.e.* before Prof. Hale's photograph was taken, the disturbance as a whole commenced at about position-angle 112° , where an eruptive prominence was situated. The material from this prominence was ejected towards the south pole, dissolving, as he describes, "from a stout, dense and bright stem into a number of bright, more or less parallel layers of striae." This appearance is shown in Prof. Hale's photograph, but when the Kensington photograph was taken it had assumed the form of concentric arches. These additional facts make me endorse Mr. Buss's opinion, that is, that it is unnecessary to assume that the material forming the arch system originated from a disturbance below it, or, as I stated in my paper, that "their concentric nature seems to suggest that they were produced at one point of initial disturbance and then moved radially outwards."

It may be added that these new facts in no way invalidate the conclusion drawn in my paper, which was that envelopes, similar in form to those photographed during eclipses, had been recorded in calcium light, thus strengthening the view that they were composed of prominence and not coronal matter.

WILLIAM J. S. LOCKYER.

Mendelism: a Personal Explanation

I SHOULD be glad if room could be found for this small matter of personal explanation. I fear I may have misled one or two of your readers on a minor point. Those of them who are interested in the interpretation of hereditary phenomena may remember that in the number of this Journal for September 12, 1907, Mr. Punnett took a reviewer to task for saying that "No one has repeated Mendel's experiments with the deliberate intention of testing the Mendelian interpretation" (of the phenomena of inheritance). In my reply I was not content with defending my original position by justifying that statement; but I must needs carry the war into the enemy's country by taking Mr. Punnett to task for not including de Vries's papers in his list of memoirs dealing with repetitions of Mendel's actual experiments, in order to show how familiar I was with the literature of the subject.

I wish to say that if I had been as familiar with the literature of the subject as Mr. Punnett was, I should not have taken the offensive. Mr. Punnett was quite right in not including a reference to de Vries's papers, because Prof. de Vries, though he has watched the results of crossing in other plants, has not worked with peas. I was misled by the commonly repeated statement that

Mendel's papers were re-discovered, and his results confirmed, at the beginning of this century, by Tschermak, Correns, and de Vries, into believing that de Vries had repeated Mendel's experiments on peas; and I rashly assailed Mr. Punnett for not making any reference to papers which I had not read. Mr. Punnett was therefore entirely in the right, and I was in the wrong.

Mr. Lock has also pointed out to me that an experiment, almost identical with the kind of crucial one which I said ought to be performed, has already been done. I am familiar with the experiment to which Mr. Lock refers (one of his own with maize), and though I regard it as very strong evidence in favour of the Mendelian interpretation of hereditary phenomena, I am sure that Mr. Lock will agree with me that an experiment the results of which will be obtained in September, 1909, is an even more crucial one.

A. D. DARBISHIRE.

THE RESEARCH DEFENCE SOCIETY.

THERE is evidence of a growing feeling among members of the medical profession that the time has come to disperse the atmosphere of mystery which has hitherto attended their ministrations, and to take the public more into their confidence as to the principles on which health may be preserved or regained. They have every reason to believe that the popular ignorance of medical science not only hinders the progress of hygiene and therapeutics, but also is the soil on which quackeries of all kinds grow and flourish, and that the education of the laity in the elementary principles of medicine would conduce to the public health, and at the same time benefit the physician by freeing him from the competition of the incompetent and unscrupulous.

One of the noisiest sections of the opponents of scientific progress is formed by the numerous small societies the object of which is stated to be the further limitation or the complete prohibition of experiments on animals. It might seem that the evidence given before the Royal Commission on Vivisection would suffice to demonstrate the benefits accruing to humanity from the use of this experimental method in the past, as well as to indicate that its prohibition would relegate medicine to the slow advance it made in the Middle Ages. But it is felt that only a small fraction of the public has the courage to seek knowledge in a Blue-book, and that to reach the multitude information must be conveyed in a less unprepossessing guise.

With this object in view, the Research Defence Society has been formed, with Lord Cromer as president, "to make known the facts as to experiments on animals in this country, the immense importance to the welfare of mankind of such experiments, and the great saving of human life and health directly attributable to them." In his letter to the Press announcing the formation of the society, Lord Cromer directs attention to the evidence given before the Royal Commission that "these experiments are conducted with proper care, and that the small amount of pain or discomfort inflicted is insignificant compared with the great gain to knowledge," and states that the society will "endeavour to make it clear that medical and other scientific men who employ these methods are not less humane than the rest of their countrymen, who daily, though perhaps unconsciously, profit by them." With this object the society proposes to publish articles, to give information to all inquirers, and assist all who desire to examine the arguments on behalf of experiments on animals.

The founders of the society ought to be gratified by the success which has already attended their efforts, for it numbers more than 1200 members, of whom 100 are ladies, and this membership has been drawn from all

departments of public life, and includes representatives of every class, including many who have taken an active part in the prevention of cruelty to animals. The medical profession is naturally largely represented, but the great number of members who appear to have no direct connection with either medicine or science indicates that there is a wide-felt impression that the methods adopted by the opponents of vivisection are objectionable, and that they have failed to justify their criticisms of this method of investigation.

The society has lost no time in opening its crusade, for we have already received two pamphlets published under its auspices. The first of these comprises "The Evidence of Lord Justice Fletcher Moulton before the Royal Commission on Vivisection" (Macmillan and Co., Ltd.), and the society is to be congratulated on having had this extremely valuable presentation of the principles of the question available as an introduction to its promised series. For, while the other witnesses on the scientific side were by the nature of things compelled to limit their evidence to a detailed account of the methods adopted in their special branches and the results accruing from them, Lord Justice Fletcher Moulton was able to take a wider view, and pointed out with indisputable logic that the experimental method offers the only way to advance in medical as in other scientific subjects. Far from questioning the justification of using animals for experimental purposes, he holds that it is actually immoral to test any method of treatment in man until it has been ascertained as far as possible by investigations on animals that it may be used without injury. As for the suggestion that investigators should experiment upon themselves, he considers that this is to be deprecated except after full investigation by means of animal experiments, not only on account of the danger to the individual subject of the experiment, but because such a procedure tends to lessen the feeling of the sanctity of human life.

The great value of Lord Justice Fletcher Moulton's evidence has been recognised by all who are interested in the subject. He was the only layman who appeared before the Commission in defence of scientific method, and he has presented his views with a cogency which must convince anyone who is capable of following a simple line of argument, and has not abandoned common sense and ordinary logic.

The second pamphlet is by Colonel David Bruce, and is entitled "The Extinction of Malta Fever (a Lesson in the Use of Animal Experiment)" (Macmillan and Co., Ltd.). It forms an admirable complement to the first, for while Lord Justice Fletcher Moulton is largely concerned with the ethical considerations involved in vivisection and the general principles of scientific investigation, Colonel Bruce gives a concise account of one case in which these principles were applied with remarkable and indisputable benefit. Malta fever formerly accounted for about 75,000 days of illness each year in the garrison at Malta, and hundreds of officers and soldiers had to be invalided to England as the result of its ravages. The old statistical methods had been applied for many years, but had failed to give any clue to the cause of the fever, and no improvement resulted from improved sanitation. Finally, the Government induced the Royal Society to send out a commission under Colonel Bruce to investigate the subject, and they soon satisfied themselves by experiments on animals that the cause of the fever is a micrococcus which gains entrance to the human body by means of the goat's milk, which is largely consumed in the island. About half the goats in Malta harboured the microbe, and 10 per cent. of them secreted it in their milk. Measures were at once taken to pre-